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III. *An Essay on the Force of Percussion,*
by William Richardson, M. D. commu-
nicated by William Heberden, M. D.
F. R. S.

Read Feb. 18, 1768. **W**HEN we consider the extraor-
dinary advancement in natural
philosophy, from the surprising discoveries of the
great Sir Isaac Newton, and other ingenious men,
who have followed his example; it may afford mat-
ter of the greatest wonder, to find the most acute
philosophers still contending, whether the force of
percussion be in proportion to the velocity of bodies
in motion, or the squares of those velocities.

Sentiments so opposite in their nature, and so
strongly supported by their respective advocates, may
well make a cautious person suspect, that nothing is
to be discovered with certainty in the operations of
nature. In which opinion he may be the more con-
firmed from this consideration, that the present
dispute is not about objects far removed from our
observation, but such actions of bodies as do con-
stantly occur to our senses, and which without dif-
ficulty may be reduced to experiment.

What then can be the reason, after such variety
of experiments have been made, why this matter is
not, before this time, brought to a decision? The

fault cannot be in the experiments themselves, the chief of which have been often repeated, and that in the most accurate manner : it must, therefore, be in the use which has been made of them, in the groundless inferences which have been drawn from them.

Those who have wrote best on this subject (whether in support of the velocities, or the squares of velocities) seem to me to have inferred more from the principle they maintain than what they bring sufficient arguments to justify ; by which means they blend truth with error, and the more they endeavour to illustrate their respective doctrines, the more they render them perplexed and confused. For the truth of what I have advanced, I appeal to the two following instances.

Those who maintain that the force of percussion is as the velocity of the striking bodies, when they account for the impressions made in soft bodies (which are found, by experiment, to be as the squares of the velocities), inform us, that the time ought to be taken into the account ; which being as the velocity of the impinging body, the impression will of course be as the time into the velocity, or (which is the same thing) as the square of the velocity.

But this, in my mind, is to assert more than what can be clearly demonstrated : for as action and reaction are equal, the more forcibly one body acts upon another, the greater must be the resistance it meets with ; and whatever be the time of its acting, supposing its force to be given, the effect produced must still remain the same.

To which I must further add, that some of the most learned and zealous advocates for time being taken into the account, have not agreed among themselves whether it be in a direct or reciprocal proportion of the velocity^a.

Those who, on the contrary, insist that the force of percussion is in proportion to the squares of the velocity, finding from experiment that in soft bodies the velocity after percussion falls short of this estimate, would make us believe, that in compressing the parts of those bodies, a certain degree of force must necessarily be lost, which, being added to what remains after percussion, will sufficiently confirm the truth of their doctrine.

To this I reply, that the parts of soft bodies are, indeed, removed out of their places by the stroke, and that some motion is lost in the impinging body, being communicated to the parts of the soft body it strikes upon; but these parts cannot lose their motion any other way, than by communicating it to other parts, or by the force accruing to the whole body.

How then are these different effects to be accounted for, and in what manner are they to be deduced from the same cause? This diversity of appearances, I have for some time suspected, might proceed from the nature of cohesion: that while the force of percussion produced an effect on the whole mass of matter which receives the stroke, in proportion to the velocity of the impinging body; it might, at the

^a Dr. Pemberton, Philosophical Transactions, N° 371. p. 57.
Dr. Clarke, Philosophical Transactions, N° 401. p. 382.

same time, in separating the cohering parts from each other, produce an effect in proportion to the square of the velocity.

Into which way of thinking I was first led from the following observations ; that a chord, which would bear a very strong pull, might easily be broken by giving it a sudden jerk ; as also that the weight of a hammer did not contribute so much in driving a nail, as the quickness of the motion given it by the driver.

In order to make a further discovery, whether or no this my supposition was really founded in nature ; I determined first to make experiments on such soft bodies as have a considerable degree of cohesion ; and then to try those bodies, when dried and reduced to powder, and by that means deprived of their cohesion ; which experiments, when compared with each other, would, I flattered myself, give me an insight into this intricate affair, and at the same time disclose that beautiful simplicity, which nature observes in all her operations.

My apparatus for making the experiments consists of four balls exactly spherical, two iron branches, and a small lead cistern.

The balls are each of them two inches in diameter ; two are of brass, and two of box-wood ; one of each sort is solid, and the other hollow ; that which is hollow is only half the weight of the solid one, and may be opened by means of a screw in the middle.

The iron branches are to give the balls their proper directions ; they have each of them a small brass pulley in the fore part, and in the hind part a kind of
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hook,

hook, which fastens them to staples at different heights; one of the branches is two inches long, and the other four inches, exclusive of pullies; by which means the balls when let fall are directed to different parts of the surface they strike upon.

The lead cistern is of an oblong form, that the matter therein contained may, at the same time, receive two distinct impressions; either when balls of different weight are let fall, or the same ball is let fall from different heights; its length is six inches, its breadth four inches, and its depth two inches.

The matter I have found best suited to the purpose is stiff clay, tempered in such a manner as to be smooth and uniform, with the same reduced to powder, after having been baked in an oven, as also after having (by a still stronger heat) been converted into brick.

The cistern is by turns filled with these different materials, which are to be closely and uniformly pressed down, so as to leave the surface quite level: in effecting which, great caution is required, more particularly in regard to the powders; as they will not distinctly retain the impressions, unless they have some small degree of moisture, or be very closely pressed down; in both which cases they acquire such a degree of cohesion, as of course must render the experiments more or less imperfect.

Things being thus prepared, in order to try the necessary experiments, I fixt the staples, and by their means the branches, at the following heights, viz two feet, four feet, and eight feet; the result of which experiments was as follows.

When

When the brass balls, in weight to each other as two to one, were let fall on tempered clay, from four feet and eight feet respectively, the impressions made were on various trials found to be equal.

When the wood balls, being to each other as two to one, were let fall from the same heights, on dried clay pulverised, that from four feet generally made the deeper impression.

When the wood balls were let fall from the same heights on brick-dust, that from four feet constantly made the deeper impression.

When the lighter brass ball was let fall on tempered clay, from two feet and eight feet, the impressions were to each other as one to four.

When the lighter wood ball was let fall on dried clay pulverised, from the same heights, the impressions were (so far as the eye could judge) nearly in the proportion of one to three.

When the same ball was let fall on brick-dust, from the like heights, the impressions were not much short of the proportion of one to two.

From these experiments it plainly appears: First, That the impressions made in soft clay are in proportion to the heights, from whence the balls are let fall, consequently as the squares of their respective velocities. Secondly, That the impressions, in pulverised clay, recede considerably from that proportion, being as it were in the medium between the squares of the velocities and the velocities themselves. Thirdly, That the impressions in brick-dust are nearly in a subduplicate proportion of the heights from whence the balls are let fall, consequently vary but

but little from the proportion of the velocities acquired.

Whence I should apprehend it clearly follows; that the impressions made in soft bodies, by hard ones striking upon them, do vary from each other, according to the degree of cohesion in the respective soft bodies; and that the impressions would be in exact proportion of the velocities, could their form be perfectly retained by bodies quite void of cohesion.

Nothing, however, being more evident to me than that actions ought to be measured by their effects, and at the same time fully depending on the accuracy of the experiments, I am determined to rest this important point entirely upon them. Shall not, therefore, attempt any illustration in the mathematical way, lest, by too far indulging a favourite opinion, I should bewilder myself in intricate calculations. Much less shall I endeavour to establish my doctrine on metaphysical principles, which seem to me in themselves too obscure, to throw any clear light on subjects of this nature.